Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-49-RC2, 2020

Biogeosciences Discussions
Open Access
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Interactive comment on "Reviews and syntheses: The mechanisms underlying carbon storage in soil" *by* Isabelle Basile-Doelsch et al.

Anonymous Referee #2

Received and published: 11 May 2020

This manuscript is a review on the mechanisms of carbon storage in soils. The aim of the review was to provide a comprehensive, in-depth overview on the scientific knowledge on processes involved in soil C dynamics, how they develop over time and how they are controlled by different biotic and abiotic factors. The authors successfully present the state of the art in these fields. The main part of both the manuscript text and the Conclusions section centers around SOM transformation processes, less emphasis is given on the (long-term) dynamics and on potential implications for soil management.

On the level of headers, the manuscript is generally well organized and addresses all fields related to the stated aims. However, my impression is that in the sections, the flow is sometimes lost and a lot of details are discussed without properly putting them into

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the large context of the review. This is even true for the Conclusions section. As a result of that, the novelty and the synthesis character of the manuscript are not really obvious. As I think this manuscript can be revised to provide a timely, necessary and relevant review, I would like to ask the authors to highlight the novel insights with respect to the aims mentioned in the introduction more in their revision of the manuscript.

I also noted that the authors give relatively statements and quite detailed information without references at a number of places in the text. The authors thus should thoroughly check if additional referencing is needed.

Here are some more detailed comments:

Abstract: The abstracts needs an introductory sentence that should go beyond the statement that recent research improved our knowledge on SOM characteristics and turnover, and a concluding sentence which refers to the aims mentioned. Similar to the whole text, the novelty and synthesis character need to be stressed more. A more formal issue is the list of bullet points, which I suggest to replace by running text, maybe with (1), (2) etc. to indicate the individual items.

Introduction: I liked the introduction very much and have no issues with it.

Main processes controlling the C stock and its dynamics in soils: This header needs rewording to better represent the content of the subsections which include information on fluxes and litter composition rather than on processes involved.

- line 54: The TOC concentrations given here do not match the ones in Figure 1, to which this sentence refers. This is because the examples given in the text include organic layers on forest soils whereas the figure uses a grassland site as an example. I also suggest to use the same units in the text and the figures (either mg/g or g/kg). Although the numbers correctly represent the range of TOC concentrations expected in soil, the authors should include reference(s) for this information.

- line 58: In addition to losses by mineralization to CO2, C might also be exported from

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soils by leaching as DOM or erosion of POM.

- lines 64-66: The return of the harvested C in form of manure or sewage sludgeis an interesting point. Is there any information or estimate available how much of the total input or of the total harvest this is?

- lines 74-90: This section should be revised for a better structure with respect to total partial fluxes. Maybe simple separation into multiple paragraphs would already solve this problem. I also suggest to resolve some seeming contradictions, such as a higher contribution via roots (line 74) while shoot biomass dominates root biomass (line 77).

- line 88: reword to read "The annual input to the soil adds up to 5.1 t C/ha/year in this example." I think it is important to keep in mind that these numbers do not apply to all soils, but that this is an example.

- line 102: Another important component of microbial input is chitin from fungal cell walls.

- line 103: replace "amino-saccharides" by "amino sugars"

- lines 105-106: I do not understand why there should be a contrast between "non-industrial organic waste products" and "compost and sewage slugde" which are certainly non-industrial organic waste products. I guess the term "non-industrial organic waste products" needs some explanation or definition.

- lines 109-111: This part has to be revised to remove a number of inconsistencies. For example, the list in the brackets "terra preta, biochar, coal mines and charcoal production sites" includes a soil type, organic material and sites. Out of these, only biochar would be a "biomass pyrolysis product". In addition, plastics do not really fit into a list about incomplete combustion products.

- line 110: replace "local organic carbon from rocks" by "organic carbon inherited from rocks"

- line 113: What exactly should be "physical and chemical biotransformation protagonists"? Both fauna and microflora are biological protagonists.

- lines 131-132: The partial sentence about the abundance of fungi and their hyphae length has to be reworded.

- line 139: The lower microbial biomass in cultivated soils usually goes along with lower TOC concentrations.

- line 150: How would growth (alone) affect substrate/enzyme contact?

- line 153: Please reword the phase about final electron acceptors and C oxidation, it combines reaction partners and processes in a strange way.

- line 158: replace "microorganisms" by "microbial"

- line 159: The production and release of extracellular enzymes does not only come with energy cost. The cells have to invest carbon and nitrogen (and sulfur), too. Also, as reactions take place outside the cells, extracellular enzyme activity is restricted to reactions which do not require energy input in form of ATP (or similar).

- line 163: Uptake of almost all organic molecules into the cells is an active process. Please replace "adsorbed" by "transported"

- line 180: This statement needs a reference.

- line 195: "Many studies" requires a number of references.

- line 199 and Fig. 2: Most depolymerization processes are hydrolytic processes

- lines 202-204: SOM carries a number of different functional groups in different chemical environments. Their pKa values cover a much wider range than pH 4-5.

- line 209: Figure 1 does not inform about the contributions of the different types of plant material mentioned in line 208.

- line 216: "elementary molecules": please reword

- line 226: Organo-mineral interactions are determined by both the availability of both the organic matter and the mineral to react with each other.

- line 226: Delete "Organic matter transfers"

- sections "OM transfers within the soil profile" and "OM transfers on the soil surface: erosion": These sections seem to be a bit misplaced within the section about transformation processes. The whole section also fails to really link well to the processes of C cycling. I also suggest to replace "transfer" by "transport"

- line 228: The context suggests "transferred through" or "transferred within"

- line 237: "10-30 mg/g/day of dry OM": The unit given here is confusing. First, "dry OM" obviously relates to the "g" and should be placed there. Secondly, is it per g plant organic matter or per g earthworm biomass (which would make more sense?)

- line 240: "hotspots of OM enriched in organic compounds" should be reworded to sound less repetitive.

- lines 236-242: How do the different ecological groups of earthworms affect C redistribution?

- line 243: "Water that circulates": Usually water movement in soil is vertical, not circular.

- line 244: I never heard of the differentiation between "leaching" and "lixivation", which I found to simply be the French word for "leaching".

- line 246: If OM sorbed to minerals is < 0.45 μm to meet the definition of DOM, the minerals must be very small, rather colloids than particles.

- lines 249-254: Additional references are required for this section.

- line 254: The DOC flux varies quite substantially. Is there more information about controlling factors?

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- line 258: Erosion is just compensated for by pedogenesis only if the soil is in steady state. If this is not the case, soil is degraded or formed.

- line 263: depleted in what?

- lines 264-265: This sentence needs rewording. Also: The effect of neglecting erosion on the C balance depends on spatial scale and how much of the eroded soil ends up in surface waters.

- section "Stabilization and destabilization of soil organic matter": Although destabilization is an important aspect for SOM turnover, it is not discussed in this section.

- lines 272-281: The sorption capacity can be simply described by the density of sorption sites and the specific surface area of the minerals.

- lines 275 and 278: "clays": The two definitions could also be reflected by using "clay particle size fraction" and "clay minerals"

- line 285: This statement is only true if sorption and degradation are in direct competition. As soil is a heterogeneous material, there sorption might be dominant in some microhabitats whereas degradation prevails in other places. In addition, spatial separation of enzymes/organisms and substrate may also limit degradation even if the substrate is not sorbed.

- lines 292-293: As chelates are usually soluble, they cannot be considered a mineral phase.

- line 300: In acid soils, Fe and Al are effective complexing ions.

- line 308-309: A simple correlation does not prove causal relationship, and certainly is not appropriate to identify cause and effect.

- lines 318-320: The term "microhabitats" should be introduced in this section. Soil heterogeneity also controls the accessibility of substrates.

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- line 331: Which information can be used for upscaling from the small scale to the plot scale?

Time dependent processes: dynamic representations:

- line 337-340: In the description of the degradation equation (Equation 1), it should considered that it is not only k which is not fixed, but also inputs are not necessarily constant.

- line 345: Isotope tracer approaches only rarely can address millennial scale processes.

- Figure 5 is somehow misleading in this context as it only shows the fate of a single input event. It ignores repeated or continuous input and thus carbon accumulation over the years. It also remains unclear if fluxes between pools were considered, and how much of each carbon fraction is transferred between the pools.

- line 354-356: rephrase to read "Finally, dating methods have shown that, in addition to OM several decades old, organic materials are partly inherited from a distant past (Mathieu et al., 2015)."

- line 366: I do not think that "mineralization" always implies "mineralization rate". It might also be used to describe cumulative mineralization over a given time or, in a slightly different field, the process of organic matter oxidation.

- line 368: replace "reserves" by "stock". One obvious reason for decoupling of mineralization rate from C stock is the availability of OM for microorganisms as a substrate.

- line 379: Just slowing down plant debris biotransformation would result in accumulation of plant residues (e.g. in peat bogs) and slow down microbial C cycling and thus accumulation of microbial necromass. It would thus greatly affect the quality of SOM. The aim must be to optimize the delicate equilibrium between using SOM as a C and nutrient source and protecting SOM in order to remove CO2 from the atmosphere and to improve soil properties.

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- line 380: "equivalent" to what?

- section "linear processes, non-linear processes": I had problems following this classification, in particular because first-order decay is classified as a linear process, while in fact it follows exponential decay kinetics. In addition, most soil C models assume a number of pools with different decay rate constants, and introduce "non-linearity" this way.

- line 393-394: "inputs, microorganisms and OM are not co-located": This statement needs to be rephrased as both inputs and microorganisms are part of OM. The message should be that the microorganisms might not be able to access a potential substrate because of spatial separation.

- lines 396-400: This section remains rather general and would profit from some more information on potential mechanisms, feedbacks etc. Some additional references might also help.

- lines 409/410 and 412: There seems to be a contradiction between these two sentences: in lines 409/410, the authors state that the lower level of priming results in longer MRT in subsoil than in topsoil, whereas in line 412, they stress that priming is particularly important in deep soil horizons. This needs to be resolved. This also applies to Table 2, where an increase in decomposable carbon input flux is assigned a negative effect on C storage.

- line 412: I think that the "risk of destabilization of pre-existing OM by carbon inputs" in deep soils is overrated by the authors. Even if it is true that C input to these horizons might result in enhanced degradation of old SOM, the net effect would always be an increase in OM because the priming effect does not completely outweigh the C input.

- line 423: As the authors want to stress how much of the C in deep soil horizons is young, I would suggest to reword to "more than 15% in permanent grasslands"

- lines 424-426: Are the responsed of the subsoil as fast as in topsoil, or does it take

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some time until the signal arrives at larger depths?

Control of C residence times in soil: biotic and abiotic factors

- lines 438-439: Although temperature clearly increases mineralization, it is not clear yet how this is reflected in SOM concentrations, as net primary production and thus litter input to soil also increases with temperature.

- line 441: 20-50% water content is rather moist. Assuming about 50% pore space, a 50% volumetric water content would correspond to saturated soil. At this water content I would already expect decreasing aerobic microbial activity.

- line 442: replace "desiccation-humectation" by "drying-rewetting"

- line 444: Fermentation is energetically extremely unfavourable for microorganisms. In the absence of oxygen, they therefore tend to use alternative electron acceptors such as nitrate, ferric iron, sulfate rather than relying on fermentation.

- line 448: Increased aeration is certainly one reason why ploughing accelerates biodegradation. Other factors such as making previously physically protected (entrapped) substrates available also contribute.

- line 449 - 452: This section needs substantial rewording. I also found the link between the very small scale (2 μm particles) and the national scale difficult to follow.

- lines 457-463: Microbial activity and community composition is clearly affected by soil pH. This should not be neglected.

- line 479: Most models use a number of pools with different degradation rate constants to describe C dynamics. This might not consider each and every mechanism, but has been found to sufficiently describe the dynamics over sufficiently long times. These models do not claim to identify factors and mechanisms, but to give quantitative estimates.

- lines 491-492: While it is true that crops are bread for maximized plant production,

this refers to above ground biomass only, and in particular to grain yield. Belowground biomass might be smaller, but seems to be important as a C input to soil, in particular to subsoils.

- Table 2 needs some revision to fill missing information. In addition, this table is merely quantitative, it does not at all reflect shifts in SOM composition/quality. As peat bogs impressively demonstrate, accumulating large amounts of OM does not necessarily result in highly productive soils.

- Fig. 4: I really like this figure as it clearly shows the relative importance of different organic matter fractions and the contribution of different organisms.

- Fig. 5: This figure represents the fate of a single input event, this should be mentioned in the caption. It should also be noted that the numbers given in the table are only valid in case of steady state. It did also not become clear to me if the pools are linked, i.e. if the C lost from the fast pool enters the intermediate and then the slow pool, or if each pool is only degraded. As the old C pool has a turnover time, it also needs some input to maintain steady state (0.002 tC/ha/yr).

- Fig. 6: Please include some information on the experiment presented here. These are data for a particular soil, they cannot be generalized.

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